# Health insurance coverage and health outcomes among transgender adults in the U.S.*

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**Abstract**

This study provides evidence of health and insurance coverage disparities between the cisgender and transgender U.S. populations using repeated cross sections from the 2014-2020 Behavioral Risk Factors Surveillance Systems. The analysis tests whether increasing the incidence of insurance coverage among transgender people could alleviate the health disparity. The empirical approach uses a fuzzy regression discontinuity design that leverages breaks in government health assistance eligibility by age. Results indicate that for transgender recipients only, insurance coverage meaningfully improves mental health of transgender recipients; for cisgender recipients only, insurance coverage reduces difficulties with concentration and memory; and for both the transgender and cisgender populations, insurance coverage contributes to important improvements in physical health, overall health, and healthcare access.

**JEL Codes:** I13, J16,

**Keywords:** Transgender, Insurance, Mental Health

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# I. Introduction

Although transgender, non-binary, and gender diverse individuals make up a non-negligible part of the population in countries around the globe, their healthcare needs and outcomes are often marginalized by practitioners and scholars alike. Not only are the number of trans and gender diverse individuals actually larger than reported in earlier studies, their healthcare needs are poorly understood. This lack of information contributes to stigma and discrimination around being transgender and also to inadequate provision of healthcare services (Winter et al., 2016). These problems in turn can lead to poor physical and mental health outcomes.

The health status of transgender and gender diverse individuals has received growing attention in the literature, often in studies that combine gender identity with sexual orientation and examine LGBT (lesbian, gay, bisexual, and transgender) people. Such studies have found that LGBT people experience higher rates of depression, anxiety, PTSD, suicidality, HIV/AIDS, and substance abuse compared with heterosexual and cisgender people (Dean et al., 2000; Graham et al., 2011; United Nations Development Programme (UNDP), 2013; Reisner et al., 2016). Studies focusing just on transgender people echo these findings (Haas et al., 2014; Hughto et al., 2015; Marshall et al., 2016; Downing and Przedworski, 2018).

The HIV/AIDS epidemic has hit transgender individuals particularly hard. A meta-analysis of the global literature found that 19% of transgender women are HIV positive, compared with 0.4% of all reproductive-age adults (Baral et al., 2013). The prevalence of confirmed HIV infections among transgender people varies considerably across countries and subpopulations and is as high as 40% (Poteat et al., 2016). Transgender individuals face discrimination and inequities in housing, employment, and access to services all over the world, all of which increase their likelihood of participating in risky sexual activity for economic reasons (Baral et al., 2013; Poteat et al., 2016; Herbst et al., 2008). Disparities in suicidality are also stark: in a large survey of 27,715 transgender people in the U.S., 40% of respondents indicated they had attempted suicide at least once in their lifetime, a rate that exceeds that of the general population by a factor of close to nine (James et al., 2016).

Health disparities between LGBT and heterosexual and cisgender people are likely rooted in minority stress, the targeting of the LGBT community by tobacco companies, and the lack of prevention and health services that adequately meet the needs of LGBT people (Washington, 2002; Meyer, 2003). In some countries, LGBT individuals might rely on family members to compensate for the lack of formal medical care, but they could still experience inappropriate care if their family members have not accepted their sexual orientation or gender identity (Padilla et al., 2007). Even if they have access to formal healthcare, LGBT

individuals may still face discriminatory treatment. For example, a World Bank survey of LGBT adults in seven Southeastern European countries found that 39% of respondents had experienced discrimination when using or trying to access healthcare services (Group, 2018). These obstacles impact not only the well-being of LGBT individuals but also have repercussions for reduced work productivity, lower labor force participation, greater public health costs, and even reduced macroeconomic growth (Badgett et al., 2019).

In the U.S., transgender adults are more likely to report poor general, physical, and mental health, and are less likely to have insurance coverage or a healthcare provider compared to their cisgender counterparts (Meyer et al., 2017). One of the biggest barriers for transgender individuals in accessing healthcare is finding a provider who is knowledgeable about transgender health issues, and another major barrier is financial constraints (Safer et al., 2016; Learmonth et al., 2018). However, very little is known about the causal impact of health insurance coverage on health outcomes among trans and gender diverse individuals. The few studies that have examined health insurance coverage among trans and gender diverse people have focused on the extent of disparities in coverage relative to cisgender individuals, but have not estimated the causal impacts of insurance coverage on health outcomes of cisgender and transgender individuals (Budge et al., 2016; Butkus et al., 2020). Our goal is to fill this gap by using data from the Behavioral Risk Factor Surveillance System (BRFSS) to examine the effect of health insurance coverage on the mental and physical health of transgender and gender diverse adults in the U.S. To the best of our knowledge, the BRFSS is the only large-scale population-based survey in the world that contains data on gender identity, health insurance, and health outcomes. These novel features allow us to not only compare health insurance and health outcomes of transgender people against a control group of cisgender people, we can also causally estimate the impact of insurance coverage on health outcomes.

What are the primary mechanisms through which the impact of health insurance on health outcomes could occur? Having health insurance makes healthcare more affordable and improves financial security, thus reducing stress. Health insurance thereby contributes to improved access to care and better utilization of healthcare services. Particularly relevant for this mechanism is access to and use of primary care and preventive services, greater use of prescription medications, treatment for substance use disorders, stricter adherence to prescribed medication schedules, and improved diagnosis and treatment of chronic diseases and conditions – including depression (Sommers et al., 2017). These types of healthcare and treatment protocols that health insurance covers are especially important for transgender individuals given the mental health issues and additional barriers to seeking care that they experience. In order to observe a causal impact of insurance coverage on health, there must be an improvement in care, not just a change in how the care is financed. Numerous observational

studies have found empirical support for this causal mechanism in the context of several health care reforms, including Medicaid and the Affordable Care Act (ACA) (Finkelstein et al., 2012; Sommers et al., 2017; Gruber and Sommers, 2019). Since unobservable characteristics such as socioeconomic status and upbringing may affect both health insurance coverage and health outcomes, simple correlations that do not account for reverse causality or omitted variable bias may be misleading. To identify the causal impact of health insurance on health outcomes, we utilize a regression discontinuity (RD) design in the context of the ACA. This approach allows us to estimate a substantive treatment effect for gender diverse individuals by leveraging age-based discontinuities in the probability of having insurance coverage on the margin of eligibility for parental coverage through the ACA or government assistance provided through Medicare. Hence we are not estimating the impact of the ACA; rather, the analysis is leveraging age-based provisions of the ACA and Medicare to estimate the causal impact of health insurance coverage.

## II. Background: The Affordable Care Act

Healthcare is far from universal in the United States. For some, healthcare is heavily subsidized, while others must bear the entire cost. Where on this spectrum Americans find themselves is based on their eligibility for federal programs– mainly, Medicare, Medicaid, and premium tax subsidies and regulations under the ACA. The ACA, which went into effect March 2010, represents the largest government reform to the U.S. healthcare system and the biggest expansion of health insurance coverage since the introduction of Medicare and Medicaid in 1965. Some of the main innovations in the law include imposing new regulations on the health insurance market (such as eliminating the ability of insurers to deny individuals on the basis of pre-existing conditions); mandating that individuals purchase health insurance or else pay a tax penalty; expanding Medicaid eligibility to all legal U.S. residents with incomes at or below 138% of the federal poverty level (FPL); providing subsidies for low-income individuals to purchase health insurance; lowering the Medicare reimbursement rates to hospitals and insurance companies; and imposing new taxes on high-income individuals to help pay for health benefits for those at the lower end of the income distribution. The share of U.S. residents without insurance coverage dropped from approximately 18% in 2010 to 10% in 2016 (the lowest in U.S. history), due in part to a strong economy but mostly to passage of the ACA (Gruber and Sommers, 2019). A review of the literature indicates not only a substantial increase in coverage, but also expanded access to healthcare, increased utilization of healthcare services, and positive impacts on health outcomes, especially maternal health

and self-reported health (Gruber and Sommers, 2019).

For the most part, eligibility for coverage under the ACA is determined by age and how far people are from the FPL. Our empirical model leverages the age eligibility threshold to identify the impact of health insurance coverage on health outcomes. Our identification strategy does not use the FPL thresholds due to endogeneity concerns as well as the low-quality income data in the BRFSS. With respect to age, the ACA allows young adults to remain on their parents' health insurance plans until their 26th birthday regardless of their ability to pay, even if they are married, are no longer living with their parents, are not declared as a dependent on their parents' tax returns, or are eligible to enroll in their own employers' plans. This provision marks a substantial expansion in coverage compared to before the 2010 ACA when many insurance companies dropped young adults from their parents' insurance plans as soon as they were no longer considered legal dependents. This age cut-off in the dependent coverage provision of the law allows us to compare outcomes for those above and below age 26 during the period of our analysis. Although the ACA does allow individuals to have two sources of health insurance, our data source only tracks whether or not someone has health insurance. Hence we are not concerned with whether someone has more than one insurance source.[1]

The standardized health insurance plans in the ACA are classified according to metal levels: bronze, silver, gold, and platinum. In the ACA marketplace, health insurance premiums are only allowed to vary by metal level, rating location, age, family size, and smoking status. Higher metal levels have a higher actuarial value and premiums. However, only silver plans have cost-sharing, which places stricter limits on out-of-pocket costs for families below 250% of the FPL. The ACA's benchmark premium is the silver plan available for the enrollee if the enrollee did not smoke. Bronze plans have the lowest premiums but have the highest out-of-pocket spending, with an actuarial value of 60%. Platinum plans, in contrast, have the highest premiums, but the lowest out-of-pocket spending, with a 90% actuarial value. The ACA tax subsidies limit the percent of family income that can be spent on the benchmark premium, irrespective of the actual metal-level enrolled in. Any income spent on premiums above the cap is reimbursed through the tax subsidy. The net benchmark premium is thus the benchmark premium minus the tax subsidy. Because of the cap schedule, the net premiums are very low for those eligible individuals below 133% of the FPL, and they steadily increase until 400% of the FPL. Those above the 400% threshold face a large increase in the net premium, which almost doubles at the subsidy cliff. In our regressions, we control for the

[1]This data limitation is not a problem in our analysis because the estimation strategy only requires a monotonic change in insurance coverage at ages 26 and 65; we do not need to know the source of that insurance.

individual's income relative to the FPL to inoculate against bias from potential discrete changes in the FPL around age thresholds.

Prior to the ACA, Medicaid eligibility was based on a combination of income, pregnancy, age, and children. The ACA replaced this system by extending Medicaid coverage to all below 138% of the FPL. However, the Supreme Court ruled that it was unconstitutional to force states to adopt the Medicaid expansion, so many states chose not to expand Medicaid, although some would choose to do so later. When the ACA was first implemented, 25 states adopted the Medicaid expansion, and as of November 2021, 39 states had done so. To encourage states to adopt the Medicaid expansion, the ACA does not give tax subsidies to citizen below the 100% of the FPL. Low-income individuals who live in states that did not expand Medicaid who are under age 64 may be in the unfortunate position of having no eligibility for any kind of government assistance for their healthcare. Furthermore, how and if Medicaid covers gender-affirming treatments is determined on a state basis. Note that our estimation strategy accounts for time-variant changes to Medicaid policy.

## III. Data description

Our analysis utilizes repeated cross sections from the 2014-2020 waves of the Behavioral Risk Factors Surveillance Systems (BRFSS). The BRFSS is an annual telephone survey conducted by the Centers for Disease Control and is currently the only nationally representative source of data on gender identity, insurance coverage, and health in the U.S. The BRFSS questionnaire focuses on measuring health behaviors and health outcomes, and it includes not only gender identity but also economic and demographic indicators such as household income, marital status, race, and education. The BRFSS questionnaire has standardized and recurring questions that are included in every survey, and also has optional modules that states can choose whether to include in their survey.

Since 2014, the BRFSS has included a Sexual Orientation and Gender Identity (SOGI) module, which has been used by 43 states at least one time since its introduction.[2] Our sample consists of cisgender and transgender men and women ages 18 and above from all states that opt into using the SOGI module. Further, we omit any respondents who do not report their gender identity, household income, age, race, employment, education, or marital

[2]Online Appendix Figure A1 reports a) the number of years each state utilized the SOGI module during the 2014-2020 period; and b) the number of years each state extended Medicaid eligibility during the 2014-2020 period. We also provide results in Online Appendix Table A1 from regressions that test if missing a SOGI module predicts Medicaid expansion. We show both the intensive and extensive margins of missing a SOGI module (that is, both module status and module years) and find that there is no statistically significant association between SOGI missingness and Medicaid expansion.

status—approximately 2.5% of the sample. The remainder of this section details how we measure gender identity, the respondents' level of poverty relative to the FPL, and health outcomes.

### III.A. Gender identity

There are more than 1 million transgender individuals in the U.S. (Meerwijk and Sevelius, 2017). In an effort to capture this population in their survey, the BRFSS asks a question on gender identity that is consistent with a measure that reflects a person's deeply held and personal self-categorization of gender (Akerlof and Kranton, 2000). The gender identity question on the BRFSS has two stages. First, the respondent is asked: "Do you consider yourself to be transgender?" If the respondent answers "Yes", then they are asked: "Do you consider yourself to be 1. male-to-female, 2. female-to-male, or 3. gender non-conforming?" If the respondent is confused about what is meant by transgender, then the interviewer says:

> "Some people describe themselves as transgender when they experience a different gender identity from their sex at birth. For example, a person born into a male body, but who feels female or lives as a woman would be transgender. Some transgender people change their physical appearance so that it matches their internal gender identity. Some transgender people take hormones and some have surgery. A transgender person may be of any sexual orientation – straight, gay, lesbian, or bisexual."

If the respondent is confused about what is meant by gender nonconforming, then the interviewer says: "Some people think of themselves as gender non-conforming when they do not identify only as a man or only as a woman." The two-part questions is advantageous since it reduces false positives, which could inundate the small transgender sample. As discussed in Carpenter et al. (2020)'s detailed analysis of transgender individuals in the BRFSS, the likelihood of such false positives is very small. The drawback is that gender identity is only asked for respondents who consider themselves to be transgender.

### III.B. Federal Poverty Level

The federal poverty level is the amount of a tax-unit's annual modified adjusted gross income (MAGI) below which that tax unit is officially considered to be living in poverty. A tax-unit includes all individuals who file taxes together on the same tax return, such as a married couple filing jointly and their dependents. MAGI refers to adjusted gross income plus supplementary social security income, nontaxable interest income, and foreign earned income and housing expenses. The FPL is published annually by Department of Health

and Human Services and is used for eligibility in many need-based federal programs. Every year, the Department of Health and Human Services publishes a base poverty threshold for a single adult and an increment that scales the poverty threshold linearly with the number of members of the tax unit.

Although our identification strategy does not utilize the FPL thresholds, we do include the FPL as a control variable. Unfortunately the BRFSS questionnaire is not adequate for measuring the FPL. The BRFSS elicits information at the household level, not the tax unit. Hence the observed number of children, number of adults, and total income in the BRFSS includes all individuals living in the same home, which may include multiple tax units. Since the FPL is based on a tax-unit's total MAGI, income measures and family size are needed at the tax-unit level, not household. We circumvent these BRFSS limitations by linking respondents to the 2015-2020 waves of the March Current Population Survey (CPS) using hot deck propensity score imputation. The March CPS, unlike the monthly CPS, includes detailed tax information from the previous calendar year. Our measure for MAGI is the total adjusted gross income for the tax-unit plus total supplementary social security income. We are unable to add total nontaxable interest income and foreign earned income and housing expenses for Americans living abroad, which implies that our measure may underestimate the tax-unit's true modified adjusted gross income.

We use this measure of MAGI in the CPS to impute the MAGI for every BRFSS respondent.[3] This process entails four steps. First, we coarsen the detailed demographic measures in the CPS so that they match the BRFSS measures, which include employment, education, age, marital status, race, household adults, household children, year, and state. Second, we append the CPS to the BRFSS and define a survey indicator that takes the value one for BRFSS observations and zero for CPS observations. Third, we regress the survey indicator on the demographic variables using a logistic link function. The predicted values of the logistic link are called propensity scores. Fourth, the FPL of each person in the BRFSS is randomly selected from one of its ten closest propensity score neighbors in the CPS. This process is called hot deck propensity score imputation (Mayer, 2013), and it is preferred to the more common regression imputation for our application. The FPL is not normally distributed and the hot deck propensity score imputation preserves the distribution, as shown in Fig 1.

[3]This imputed MAGI is used to construct the FPL control variable in our analysis. Using a rough measure of the FPL based only on BRFSS data on income and household size does not alter the regression results, neither does omitting FPL controls.

### III.C. Health outcomes

A total of eight health outcomes were selected from the BRFSS for the empirical analysis. All are self-reported, and for each outcome we code respondents who do not know the answer or refuse to respond as missing. Although self-reported health could be critiqued as not being an objective health measure, Becchetti et al. (2018) finds that self-assessed health is a robust leading predictor of changes in actual health outcomes. A number of previous economic studies have used the BRFSS self-assessed health measures (Carpenter et al., 2020), and others have determined that the BRFSS questionnaire is a valid and reliable tool to assess mental and physical health. In particular, BRFSS measures of most health outcomes have similar patterns compared to other major health surveys, and the self-reported measures of mental health and overall health have substantial reliability (Hsia et al., 2020; Pierannunzi et al., 2013). The first health outcome assesses mental health. Respondents are asked: "Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?" In the analysis, this variable is labeled as *Mental days* and is defined as the number of bad mental health days in the past 30 days. The second outcome assesses physical health. Respondents are asked: "Now thinking about your physical health, which includes physical illness and injury, for how many days during the past 30 days was your physical health not good?" We label this outcome as *Physical days*, defined as the number of bad physical health days in the past 30 days.

The next two outcomes both assess overall health, with the first constructed as a linear variable for the number of days of bad overall health and the second as a dummy variable for whether or not the person considers themselves to be in poor health. For the number of bad health days, respondents are asked: "During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?" This outcome is labeled as *Bad days* and is defined as the number of bad physical or mental health days reported. The dummy variable for poor overall health comes from the question: "Would you say that in general your health is — 1 Excellent 2 Very Good 3 Good 4 Fair 5 Poor." The variable is labeled as *Poor health*, taking the value one if the respondent reports having poor health and zero if the respondent reports having better than poor health.

The fifth outcome assesses the ability to access healthcare due to cost. Respondents are asked: "Was there a time in the past 12 months when you needed to see a doctor but could not because of cost?" We defined the dummy variable *Cost*, which takes the value one for yes and zero for no. The sixth outcome is a measure for difficulty concentrating due to health, and it comes from the question: "Because of a physical, mental, or emotional condition, do

you have serious difficulty concentrating, remembering, or making decisions?" We created the dummy variable *Concentration*, which takes the value one for yes and zero for no. The seventh outcome measures difficulty doing errands due to health, and it is generated from the question: "Because of a physical, mental, or emotional condition, do you have difficulty doing errands alone such as visiting a doctor's office or shopping?" The dummy variable *Errands* takes the value one for yes, zero for no. The last health outcome measures depressive disorder and it comes from the question: "(Ever told) you have a depressive disorder (including depression, major depression, dysthymia, or minor depression)?" The dummy variable *Depression* takes the value one for yes, zero for no.

## IV. Empirical model

This study employs a regression discontinuity design, which is a quasi-experimental research design that identifies the causal effect of an intervention assigned at a threshold. By focusing on observations that fall closely along either side of the threshold, one can estimate an average treatment effect in a context when complete randomization is not possible (Thistlethwaite and Campbell, 1960). In our analysis, the expanded coverage of health insurance to young adults up through their 26th birthday under the ACA implies that young adults up through the age of 26 who may not have had health insurance before the ACA now had health insurance coverage. The ACA mandate implies a fall in coverage for young adults who age off their parents' plans (Fig 2). Similarly, since Medicare is only available for ages 65 and older without a specific condition, there is spike in the coverage at age 65 (Fig 3). Our identifying assumption for the reduced-form estimates is that these two cohorts, slightly younger or older than age 26 or 65, do not exhibit any systematic differences other than their exposure to the policy. As long as this assumption holds, this approach represents a treatment assignment that is as good as random. We also assume that the effect of losing coverage shares the same properties as gaining coverage. That is, we use a 'reverse' logic to say that the effect of gaining insurance operates in the same way as the opposite of losing coverage. Although most previous studies of health insurance impacts on health outcomes have examined expansions in insurance coverage, there is some available evidence to support our assumption about losing coverage (Burstin et al., 1998; Kasper et al., 2000; Mestres et al., 2021).

At each of the above thresholds, there is a naturally occurring experiment. If unaccounted health determinants are not discontinuous at the threshold and eligibility for the program has a meaningful impact on insurance coverage, then comparing people just above and just

below the threshold gives quasi-random assignment of insurance coverage, allowing us to estimate the causal impact of insurance coverage on health outcomes.

While a regression discontinuity design has the potential to eliminate the endogeneity of insurance coverage, there are two major drawbacks. First, since the sample only includes respondents in a small bandwidth around the threshold, the sample size may be small, making it very unlikely that we could detect the impact of insurance with the small transgender sample. Second, estimates from a regression discontinuity design are only valid for observations near the threshold, thus limiting external validity. We circumvent these issues by pooling the above thresholds with a stacked-fuzzy regression discontinuity design, as follows:

$$\begin{aligned} Y_{id} = \beta I_{id} + \sum_{d=1}^{2} \Big( Z'_{id}\alpha_d + D_{id}\gamma_d + r_p(X_{id} - \bar{x}_d)'\delta_d + r_p\Big(D_{id}(X_{id} - \bar{x}_d)\Big)'\eta_d \Big) \\ + \mu_{std} + \epsilon_{id} \end{aligned} \tag{1}$$

$$\begin{aligned} I_{id} = \sum_{d=1}^{2} \Big( Z'_{id}\theta_d + D_{id}\kappa_d + r_p(X_{id} - \bar{x}_d)'\lambda_d + r_p\Big(D_{id}(X_{id} - \bar{x}_d)\Big)'\xi_d \Big) \\ + \omega_{std} + \nu_{id} \end{aligned} \tag{2}$$

where $Y$ is a health outcome of interest for respondent $i$ in state $s$ during year $t$ within the bandwidth of the discontinuity threshold $d$. The notation $I$ indicates having insurance coverage, and $Z$ is a vector of controls. The regression includes state-year-discontinuity threshold fixed effects $\omega$ and $\mu$, which control for all time-invariant selection at the state level and all time-variant selection that is stable among all respondents in a state. The regression includes a polynomial degree $P$ of the running variable age $X$ centered at the threshold $\bar{x}$, and $T$ is an indicator for the running variable R being at or above zero. The impact of health insurance coverage is identified with the jump in the running variable $T_d$. The control variables include survey type, marital status-LGB interaction, number of children in the household, number of adults in the household, pregnancy, race, education-employment interaction, and FPL quintiles. Using control variables for regression discontinuity designs is a common practice that has two justifications (Calonico et al., 2019). First, these controls can increase the precision of the estimates as long as they are not discontinuous at the thresholds of our RD design. Second, if the discontinuity in the running variable impacts the outcome through a channel other than treatment, then a fuzzy RD design is not valid unless the alternative channel is controlled for.[4]

[4]Online Appendix Figures A6-A10 test for such alternative channels. Specifically, we test if key determinants of insurance coverage or health are discontinuous at age 26 and 65 – education, marital status, employment, and FPL. A discontinuity in an observable correlate of insurance coverage or health would raise suspicion of unobserved associations, tarnishing the credibility of the research design. All of these variables do not

The baseline estimates use a degree one polynomial and a triangular kernel with the mean-squared error optimal bandwidth, which is a common practice and has the best asymptotic properties (Cattaneo et al., 2019). The kernel weights imply local estimates, and the estimation with polynomial degree one implies linear. The mean-squared error optimal bandwidth is used to optimize the well-known variance-bias tradeoff in RD designs (Calonico et al., 2014). For each health outcome, our stacked estimate is the OLS-weighted average across the two local average treatment effects at age 26 and age 65, and the weights correspond to the sample size and variance of the estimate. Note that we also provide estimates using higher order polynomials, alternative kernels, and smaller bandwidths. The local linear regressions are estimated using weighted two-stage least squares (2SLS), a common procedure for estimating fuzzy RD designs, as follows:

$$(\hat{\beta}, \hat{\alpha}, \hat{\gamma}, \hat{\delta}, \hat{\eta}, \hat{\mu}) = argmin_{\theta,\kappa,\lambda,\xi,\omega}\Big\{Y_{id} - \sum_{d=1}^{2}\Big(Z'_{id}\alpha_d + D_{id}\gamma_d + r_p(X_{id} - \bar{x}_d)'\delta_d + r_p\Big(D_{id}(X_{id} - \bar{x}_d)\Big)'\eta_d\Big) - \beta\hat{I}_{id} - \mu_{std}\Big\}^2 K_{h_d}(X_{id} - \bar{x}_d) \tag{3}$$

$$(\hat{\theta}, \hat{\kappa}, \hat{\lambda}, \hat{\xi}, \hat{\omega}) = argmin_{\theta,\kappa,\lambda,\xi,\omega}\Big\{I_{id} - \sum_{d=1}^{2}\Big(Z'_{id}\theta_d + D_{id}\kappa_d + r_p(X_{id} - \bar{x}_d)'\lambda_d + r_p\Big(D_{id}(X_{id} - \bar{x}_d)\Big)'\xi_d\Big) - \omega_{std}\Big\}^2 K_{h_d}(X_{id} - \bar{x}_d) \tag{4}$$

where $r_p(x) = (1, x, .., x^p)'$,$K_{h_d}(u) = K(u/h_d)/h_d$,$K()$ is a kernel function, and $h$ is the bandwidth for discontinuity $d$. The estimator assumes $\gamma_d$ is zero. Standard errors are clustered by primary sampling unit and sampling weights are applied. All regressions are estimated with the user-written command `reghdfe` in STATA (Correia, 2017). Note that the transgender sample around each threshold is very small, which is a problem since a weak first stage will bias the estimates toward the weighted least squares estimates of the second stage. To alleviate this concern we also estimate our model using Limited Information Maximum Likelihood (LIML) instead of 2SLS. LIML has better small sample properties and has better performance with weak instruments, but LIML is less precise than 2SLS. Our main results table reports both sets of estimates.

This model requires three assumptions. First, the instrumental variables are *relevant*, meaning they must have a meaningful impact on insurance coverage, such that $\kappa_d \neq 0$.

have meaningful discontinuities at either age 26 or 65 when a small bandwidth is used (save a small yet statistically significantly drop in employment at age 65). To further ensure such alternative channels are closed, our baseline specification controls for major ways individuals may obtain insurance coverage; college enrollment (education), a partner (marital status), employer-provided insurance (employment), Indian Health Service (race), Medicaid (FPL and state-year fixed effects), or the ACA marketplace (FPL and state-year fixed effects).

Second, the instrumental variables must be *independent*; that is, they are uncorrelated with any unobserved confounders of the relationship between insurance coverage and the health outcome. Third, the instrumental variables are *excludable*, they cannot impact the health outcome through any other channel than health insurance coverage conditional on the controls, such that $\gamma_d = 0$. If these three assumptions are satisfied and the impact of health insurance coverage is homogenous, then the estimates uncover the average treatment effect of insurance coverage on the health outcome. If the impact of health insurance coverage is not homogenous and the assumptions are satisfied, then the estimates represent a weighted average of the local average treatment effect of health insurance coverage around each threshold on the health outcome, as long as the instrumental variables have a monotonic impact on insurance coverage. Specifically, the pooled estimate is a double-average, giving high weight to cutoffs that are most likely to occur and cutoffs that have many observations near the threshold (Cattaneo et al., 2016).

# V. Results

## V.A. Descriptive statistics

Table 1 reports descriptive statistics for our sample. The transgender population (binary) is small, less than 0.5% of the sample, and has some striking health and demographic differences with the cisgender population.[5] Transgender respondents report having 6.73 bad mental health days over the past 30 days, almost twice as many as the cisgender population. Moreover, while the difference is not as large, transgender respondents report having 1.08 more bad physical health days and 2.06 more bad health days in general than the cisgender population. Furthermore, a relatively larger share of transgender respondents report having poor or fair health, and 32% of the transgender sample has a diagnosed depressive disorder, almost doubling the cisgender rate of depression. Some of the health differential may stem from the relatively lower rate of insurance coverage, which is 5 percentage points below the cisgender sample. This differential in coverage is slightly smaller than the employment gap (45% for transgender versus 52% for cisgender) but considerably smaller than the differential in high school dropout rates (25% versus 12%), non-white racial/ethnic status (46% versus 36%), LGB status (33% versus 5%), and being married (43% versus 53%). These correlates of socioeconomic status could also drive health outcomes and are thus included in our regression

[5]Nonbinary transgender respondents are omitted due to a priori heterogeneity in gender-affirming care, as there is no standardized hormonal treatment protocols for non-binary transgender individuals (Cocchetti et al., 2020).

model.

The relatively poor health of the transgender sample has consequences. About one fifth of the transgender sample has difficulty concentrating due to a physical, mental, or emotional condition, which doubles the cisgender rate; 15% of the transgender sample has difficulty doing errands for the same reasons, exceeding the cisgender rate by more than a factor of 2. Yet the transgender sample is around four years younger than the cisgender sample on average.

Some of the health differential may be behavioral. The transgender sample binge drinks almost twice as many days as the cisgender sample, but the difference in drinking days per week is negligible. Notably, 14% of the transgender sample smokes cigarettes every day and 6% smokes cigarettes some days; both rates are around two percentages points above the cisgender sample.

### V.B. First-stage and reduced-form estimates

Recall that our analysis draws two comparisons around each threshold (age 26 and 65): first, the health outcome for people slightly younger versus slightly older (reduced form estimate); and second, the insurance rate for people slightly younger versus slightly older (first stage estimate). The final estimate is the change in the health outcome across the threshold divided by the change in the insurance coverage rate across the threshold (2SLS). The 2SLS estimate assumes that exposure to policy at the threshold is responsible for the change in both insurance coverage and health. Furthermore, the 2SLS estimate assumes health only changed because of the policy-altered insurance coverage. To be clear, the sample includes people with and without insurance, slightly above and below each threshold.

Table 2 reports the first-stage and reduced-form estimates for each outcome. This discussion focuses on the first stage estimates for the sake of brevity. Since the research design assumes that the change in insurance coverage at the threshold is meaningful, nonzero, and the sign is known a priori, the first-stage estimates ascertain the validity of the research design at each threshold. There is a meaningful jump in insurance coverage at age 65, which is excepted since the respondent becomes eligible for Medicare. At age 26, it is likely that a young adult will age off their parents' health insurance since coverage is no longer required by the ACA, which is reflected in the negative first-stage estimates. Overall these estimates support the strength of the instruments used in the stacked 2SLS approach.

### V.C. Impact of insurance coverage on health outcomes

The descriptive statistics confirm what has become an unfortunate stylized fact, namely that the transgender population has relatively worse health than the cisgender population, especially mental health, along with lower insurance coverage. This section digs deeper into these patterns by reporting the benchmark estimates of the impact of insurance coverage on each health outcome. Could expanding transgender insurance coverage alleviate the substantial disparities in health?

Fig 4a depicts the benchmark estimates for the impact of insurance coverage by transgender status on self-reported bad health days (mental, physical, or either) over the past 30 days. Because the 2SLS estimations are preferred over LIML for potentially having less bias, the results underlying this figure are reported in Table 3 as the 2SLS results. Note that the LIML results in Table 3 are very similar in magnitude and precision as the 2SLS results with one small exception: the coefficient on insurance coverage for the transgender sample is loses its 10% statistical significance in the LIML estimation in model 14 (for running errands). Although the small size of the transgender population poses a hurdle to estimating the impact of insurance coverage with precision, the results do show that insurance coverage has a meaningful role. In particular, insurance coverage for the transgender sample reduces the number bad health days by 15.6 (s.e.=7.5), the number of bad physical health days by 15.0 (s.e.=6.14), and the number of bad mental health days by 23.7 (s.e.=9.37); these impacts are each statistically significant at over 95% confidence. Like the transgender sample, insurance coverage also has a meaningful impact on cisgender bad health days and bad physical days, but not on bad mental health days.

Fig 4b displays the benchmark estimates for the impact of insurance coverage by transgender status on the five binary health outcomes. If cisgender, insurance coverage reduces the probability of not being able to see a doctor due to cost by 39.8 (s.e.=7.3) percentage points, and it reduces the probability of having poor health in general by 14.3 (s.e. = 5.7) percentage points. In addition, the cisgender probability of having problems with concentration and memory also shows a substantial decline. The health improvements from having insurance coverage are even larger for the transgender sample in terms of magnitude, although they are measured with somewhat less precision due to the small sample size. In particular, transgender individuals who get insurance coverage experience a 31.2 (s.e.=16.4) percentage point fall in the likelihood of reporting poor general health, and a 65.8 (s.e.=32.7) percentage point decrease in the likelihood of not being able to see a doctor due to costs. Both these results are striking. Transgender individuals also see a meaningful decline in the probability of having difficulty doing errands because of a health condition, while this estimate is not estimated with precision for cis-gender individuals.

Taken together, the results suggest insurance coverage makes medical care more accessible and improves the general health of enrollees. Both transgender and cisgender enrollees experience sizeable improvements in physical health, overall health, and access to medical care. Improvements in mental health and the ability to perform errands are seen by transgender individuals alone, while cisgender people alone see improvements in concentration as a result of insurance coverage. The positive effects of health insurance coverage are consistent with estimates of insurance effects on self-reported physical and mental health for the overall population reported in Finkelstein et al. (2012)'s seminal study of Oregon's Medicaid expansion and with those summarized in Gruber and Sommers (2019) for the Affordable Care Act.

### V.D. Robustness

Results from a battery of robustness tests are reported in Online Appendix Figures A2-A5 and Tables A2-A4. In the spirit of Leamer (1983), we conduct a sensitivity analysis of the benchmark estimates, providing estimates for all of the feasible choices we could have made additively along the way to obtaining the benchmark estimates. In this process, the set of choices are constrained to those within our research design à la Angrist and Pischke (2010). Specifically, we gauge the sensitivity of the benchmark estimates to the use of 2020 data, choice of kernel, bandwidth, polynomial, estimator, omission of thresholds, and set of controls. Overall, the findings for mental health, physical health, and health care access are robust. However, the findings for general health improvements are sensitive to the use of 2020 data, kernel choice, polynomial of the running variable, and bandwidth.

## VI. Discussion

This study has shown that cisgender individuals are more likely to have health insurance than transgender individuals. These differences in health insurance coverage warrant close attention in order to devise appropriate remedial policies, especially because receiving gender-affirming care is likely to be more cost-effective in the long run relative to a health insurance system that leaves relatively more transgender people uncovered and without treatment (Learmonth et al., 2018). Results from the regression discontinuity approach offer compelling evidence that health insurance coverage has a positive impact on several health outcomes, especially for the mental health of transgender people. More specifically, results for transgender individuals show a causal impact of insurance coverage on a reduction in bad mental health days, bad physical health days, and bad overall day each month. Transgender people also experience reductions in poor health and improvements in access to doctors and ability to

perform errands. With the exception of improvements in mental health, most of these health improvements are also found for the cisgender population, but the magnitudes of the effects are somewhat smaller. Given that transgender individuals are less likely to have health insurance coverage and less likely to have a primary care doctor than cisgender people (Budge et al., 2016), it is not surprising to find a larger impact of insurance coverage on key health outcomes for transgender compared to cisgender individuals.

Why would we not see an impact of insurance coverage on the mental health of cisgender individuals given that insurance has a causal effect on healthcare access and utilization? A likely explanation is that transgender individuals have higher rates of depression, anxiety, and other mental health issues than cisgender individuals (Downing and Przedworski, 2018), with the implication that transgender people are more likely than their cisgender counterparts to use healthcare services and prescription medications to treat mental health issues. Another explanation is that many Medicaid and private health plans exclude gender-affirming hormone and surgery care (Gorton, 2007). Because Medicare does include gender-affirming care, some of our positive effect of health insurance on mental health may be picking up access to gender-affirming care. If transgender people were able to use their previous health insurance to cover the costs of preventive care and treatment for their physical health but not for some of their mental health needs, this would help to explain why their self-reported mental health shows a stronger positive response to insurance coverage through Medicare compared to self-reported physical health and overall health.

Discontinuous changes in the quality of health insurance for the transgender population warrant some caution when interpreting the 2SLS estimates. People with insurance may gain access to gender-affirming care through Medicare if they were not covered by their prior provider. If this occurs, then our approach overestimates the effect of insurance coverage. We would overestimate the effect of insurance coverage by misattributing the gain in health from gender-affirming care to the already insured at the discontinuity to the change in insurance coverage at the discontinuity. Unfortunately we cannot test for this problem without panel data, so instead we turn to research on the magnitude of this issue. Data from the 2015 U.S. Transgender Survey indicate that 86% of respondents were covered by health insurance (including 5% covered by Medicare), and of those covered respondents, 25% were denied coverage for transition-related hormone therapy, and 55% were denied coverage for transition-related surgery (James et al., 2016). Thus such a misattribution of changes in the quality of insurance to gains in coverage are entirely feasible for the 2SLS estimates. Yet, this problem does not threaten the policy relevance of our findings, because the reduced-form estimates are still valid barring any simultaneous birthday-effects. Regardless of this problem, our findings show expansions to Medicare or parental coverage under the ACA would meaningfully

improve the health of the transgender population.

An important policy lever to address health disparities between transgender and cisgender people is health insurance coverage. Numerous studies have linked insurance coverage with access to care and with improved health outcomes in countries around the world (Erlangga et al., 2019; Sommers et al., 2017). In fact, achieving universal healthcare and access to quality services is one of the health targets in the Sustainable Development Goals for 2030. People lacking health insurance often slide into poverty due to disproportionately high out-of-pocket expenses during health episodes. In some low-income countries, up to 11% of the population faces severe financial hardship due to out-of-pocket healthcare costs, and up to 5% of the population falls into poverty because households have to pay for healthcare costs (World Health Organization, 2010).

Our results build on previous evidence based on a sample of transgender and gender diverse individuals showing a positive association between having health insurance and seeing a mental health therapist or a psychiatrist (Carter et al., 2020). Similarly, Christian et al. (2018) found a positive association between having access to a transgender-inclusive health provider and having less depression and fewer suicide attempts in a sample of transgender individuals. Closely related, evidence in Goldenberg et al. (2020) indicates that transgender individuals living in states with more protective legislation for transgender people around health insurance coverage and anti-discrimination had a lower odds of avoiding healthcare. More broadly, several studies based on different samples of LGBT people have found that lack of health insurance poses a major barrier in seeking healthcare (e.g. Qureshi et al., 2018; Stepleman et al., 2019). Our causal estimates bolster the arguments in these previous studies regarding the importance of health insurance coverage for improving the mental and physical health of transgender individuals and promoting overall health equity.

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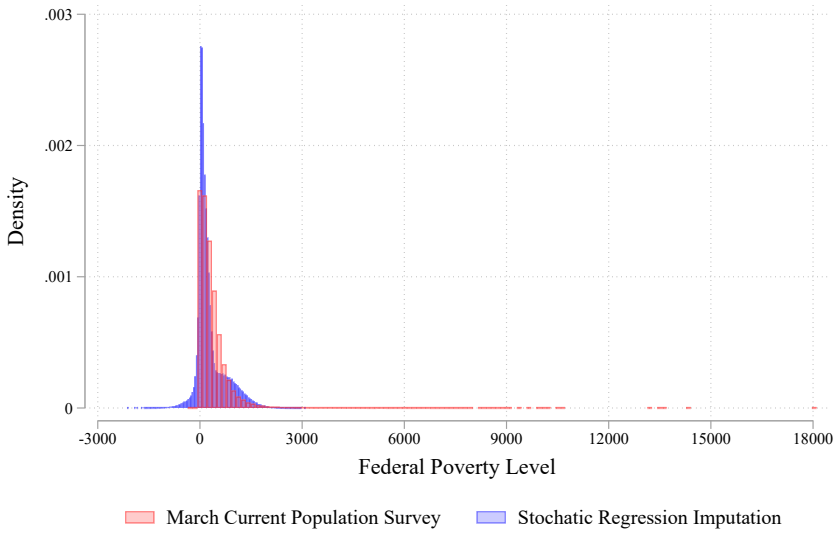
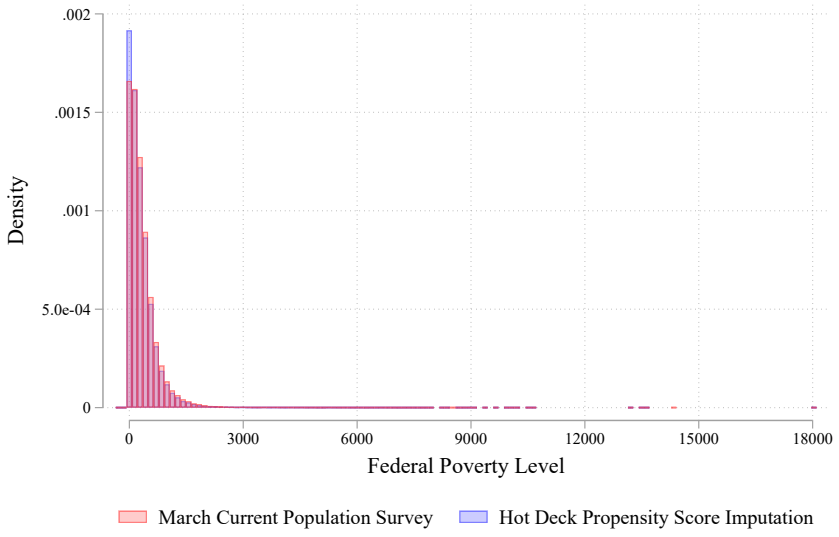
Figure 1: Histograms Comparing Distribution of Imputed and Observed Federal Poverty Level by Imputation Method



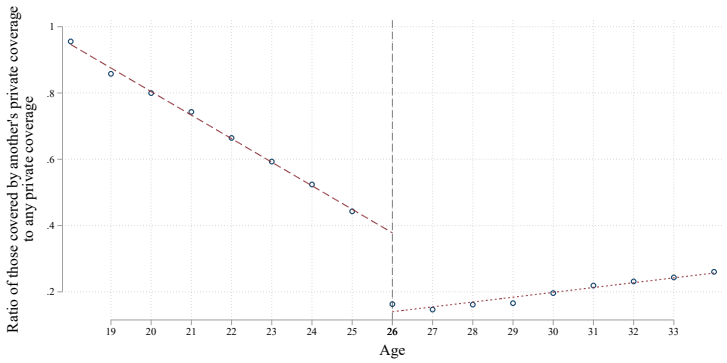

(a) Single Stochastic Regression Imputation



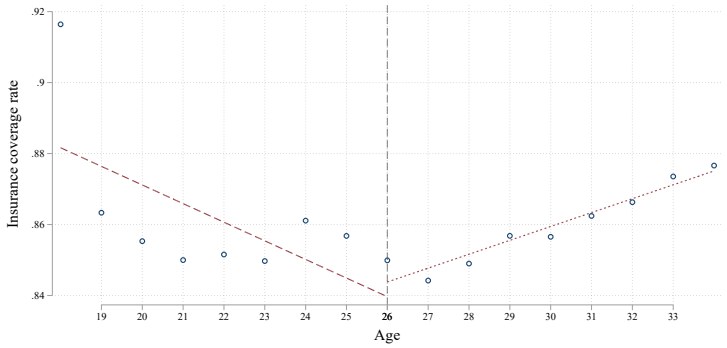

(b) Single Hot Deck Propensity Score Imputation

*Notes:* Figures compare the distribution of the Federal Poverty Level (FPL) in the March CPS to the imputed FPL in the BRFSS, sampling weights are applied. Figure 1a uses single stochastic regression imputation. Using the pooled March CPS, the FPL is regressed on for employment, education, age, marital status, race, household size top coded at 10, a dummy for household size top-coding, year, and state. The parameter estimates are next used to predict the FPL for the BRFSS sample. The residual estimate is then added to the prediction, which is normally distributed with mean zero and variance equal to the mean squared error of the regression. Figure 1b displays the the imputed distribution from a single Hot Deck Propensity Score (HDPS) imputation; we append the CPS to the BRFSS and regress a survey indicator on the above controls with a logistic link function. Each BRFSS observation is grouped with the ten CPS observations that have the closest predicted value, ties are broken randomly. The BRFSS observation's imputed FPL is a random draw from these ten CPS observations.

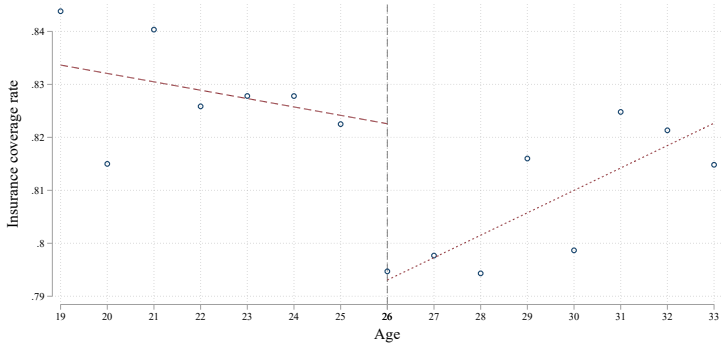
Figure 2: Binscatter Plots of the Parental Coverage Discontinuity at Age 26




(a) Binned-average Private Coverage from Another Ratio by Age (CPS)



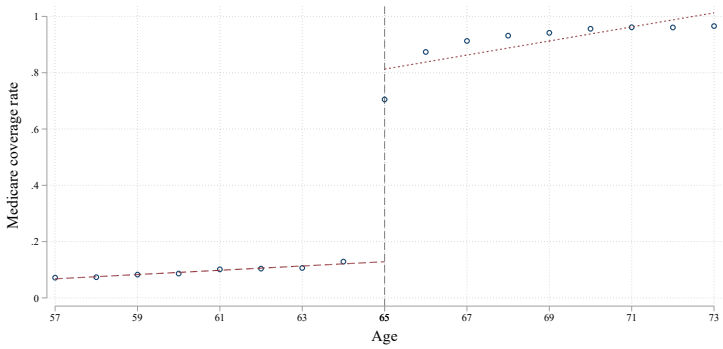

(b) Binned-average Insurance Coverage Rate by Age (CPS)



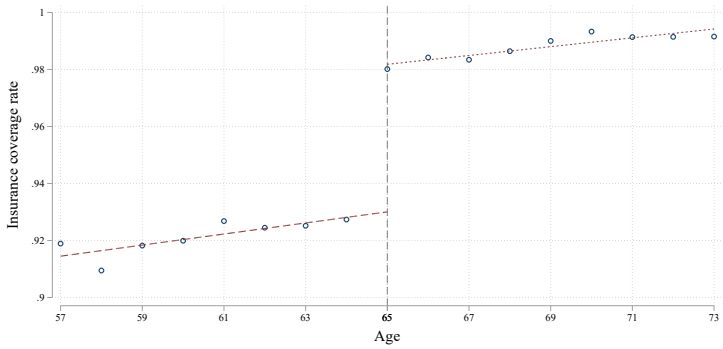

(c) Binned-average Insurance Coverage Rate by Age (BRFSS)

*Notes:* Blue dots represent binned averages where the bins are quantiles. The red line is the least squares regression, allowing for a break and kink at the dashed vertical line. Sample weights were applied. Bandwidths are the MSE-optimal bandwidths for the benchmark specification for the transgender sample.

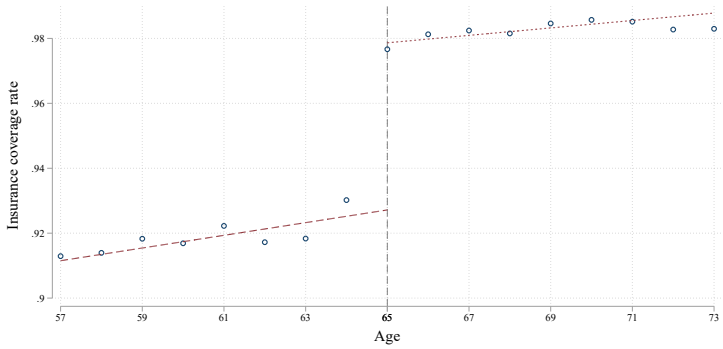
Figure 3: Binscatter Plots of the Medicare Coverage Discontinuity at Age 65




(a) Binned-average Medicare Coverage Rate by Age (CPS)



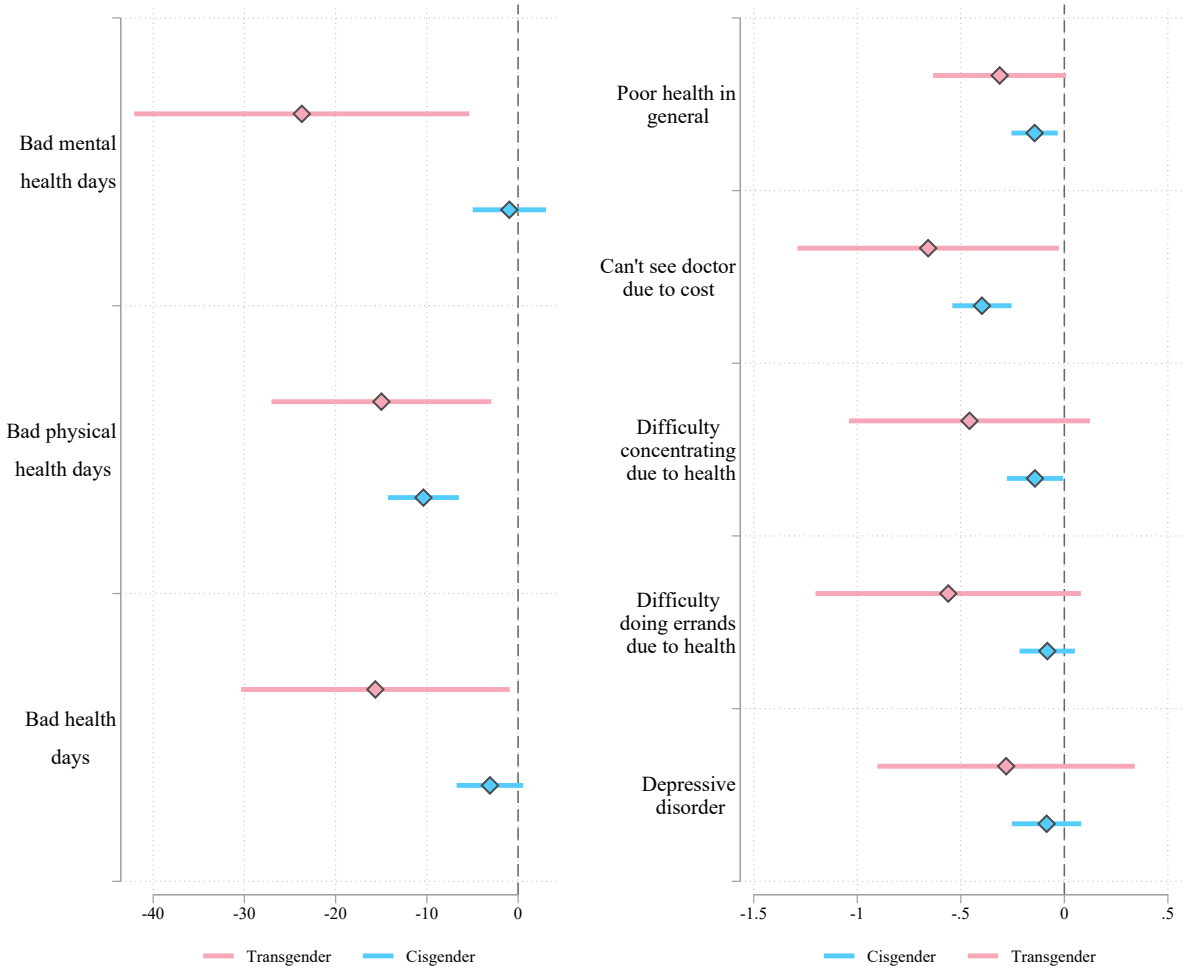

(b) Binned-average Insurance Coverage Rate by Age (CPS)




(c) Binned-average Insurance Coverage Rate by Age (BRFSS)

*Notes:* Blue dots represent binned averages where the bins are quantiles. The red line is the least squares regression, allowing for a break and kink at the dashed vertical line. Sample weights were applied. Bandwidths are the MSE-optimal bandwidths for the benchmark specification for the transgender sample.

Figure 4: Impact of Health Insurance on Self Reported Health Outcomes by Transgender Status




(a) Bad health days over last thirty days

(b) Binary outcomes

*Notes:* Figure displays our benchmark estimates for the impact of insurance coverage on select health outcomes by transgender status. The marker denotes the estimate and the horizontal lines shows the 95% confidence intervals, which are based on standard errors that are clustered by primary sampling units. Sampling weights are applied. The benchmark estimates are the stacked two-stage least squares estimates with a triangular kernel, stacking the jump discontinuities at ages 26 and 65. All estimates include state-year fixed effects and control for an education-employment interaction, marital status-lgb interaction, race, number of children in household, number of adults in household, pregnancy, survey type, and FPL. The bandwidth is the transgender MSE-optimal bandwidth for each discontinuity. All variables other than insurance coverage are interacted with a categorical variable enumerating the discontinuities.

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Table 1: Descriptive Statistics by Transgender Status, BRFSS 2015-2020

| | (1) All | (2) Cis | (3) Trans |
|---|---|---|---|
| *Mean / (Standard Deviation):* | | | |
| Bad mental health days (over past 30) | 4.02 | 4.01 | 6.73 |
| | (8.1) | (8.09) | (10.05) |
| Bad physical health days (over past 30) | 3.73 | 3.72 | 4.8 |
| | (8.13) | (8.12) | (8.75) |
| Bad health days (over past 30) | 2.55 | 2.54 | 4.6 |
| | (6.8) | (6.79) | (8.52) |
| Insurance coverage (1 = covered, 0 = uncovered) | .88 | .88 | .83 |
| | (.32) | (.32) | (.38) |
| Federal poverty level (income / poverty line $*100$) | 318 | 318 | 325 |
| | (441) | (441) | (407) |
| Age (18 - 80+) | 47.05 | 47.07 | 43.49 |
| | (16.45) | (16.45) | (17.5) |
| Binge drinking (days per week) | .18 | .18 | .28 |
| | (.73) | (.73) | (1.1) |
| Drinking (days per week) | 1.15 | 1.15 | 1.01 |
| | (1.85) | (1.85) | (1.84) |
| Cellphone survey (1 = cellphone, 0 = landline) | .69 | .69 | .73 |
| | (.46) | (.46) | (.44) |
| *Percentages:* | | | |
| Self-reported health | | | |
| Excellent | 19 | 19 | 18 |
| Very good | 33 | 33 | 25 |
| Good | 31 | 31 | 37 |
| Fair | 13 | 13 | 14 |
| Poor | 4 | 4 | 6 |
| Missing/refused | 0 | 0 | 0 |
| Couldn't see doctor due to cost | | | |
| Yes | 13 | 13 | 22 |
| No | 87 | 87 | 78 |
| Missing/refused | 0 | 0 | 0 |
| Difficulty concentrating | | | |

Table 1: Descriptive Statistics by Transgender Status, BRFSS 2015-2020

| | (1) All | (2) Cis | (3) Trans |
|---|---|---|---|
| Yes | 10 | 10 | 22 |
| No | 89 | 89 | 75 |
| Missing/refused | 1 | 1 | 3 |
| Difficulty doing errands | | | |
| Yes | 6 | 6 | 15 |
| No | 93 | 93 | 82 |
| Missing/refused | 1 | 1 | 4 |
| Depressive disorder | | | |
| Yes | 19 | 19 | 32 |
| No | 81 | 81 | 67 |
| Missing/refused | 0 | 0 | 1 |
| Body-mass-index group | | | |
| Underweight | 2 | 2 | 3 |
| Normal weight | 29 | 29 | 29 |
| Overweight | 34 | 34 | 31 |
| Obese | 31 | 31 | 29 |
| Missing/refused | 5 | 5 | 8 |
| Current smoking status | | | |
| Every day | 11 | 11 | 14 |
| Some days | 5 | 5 | 6 |
| Not at all | 82 | 82 | 76 |
| Missing/refused | 1 | 1 | 4 |
| Employment | | | |
| Employed for wages | 52 | 52 | 45 |
| Self-employed | 10 | 10 | 10 |
| Out of work for 1 year or more | 2 | 2 | 5 |
| Out of work for less than 1 year | 3 | 3 | 4 |
| A homemaker | 6 | 6 | 5 |
| A student | 5 | 5 | 8 |
| Retired | 16 | 16 | 12 |
| Unable to work | 7 | 7 | 11 |
| Education | | | |
| Did not graduate high school | 12 | 12 | 25 |
| Graduated high school | 27 | 27 | 33 |
| Attended college | 32 | 32 | 27 |
| Graduated from college | 29 | 29 | 15 |
| Race | | | |

Table 1: Descriptive Statistics by Transgender Status, BRFSS 2015-2020

| | (1) All | (2) Cis | (3) Trans |
|---|---|---|---|
| White only, Non-Hispanic | 64 | 64 | 54 |
| Black only, Non-Hispanic | 12 | 12 | 13 |
| Other, Non-Hispanic | 8 | 8 | 13 |
| Hispanic | 16 | 16 | 20 |
| Marital Status | | | |
| Married | 53 | 53 | 43 |
| Divorced | 11 | 11 | 11 |
| Widowed | 5 | 5 | 4 |
| Separated | 3 | 3 | 4 |
| Never married | 23 | 23 | 32 |
| Umarried couple | 5 | 5 | 6 |
| Sexual Orientation | | | |
| Straight | 93 | 94 | 64 |
| Lesbian, gay, or bisexual | 5 | 5 | 33 |
| Missing/refused | 1 | 1 | 3 |
| Sample size | 1,056,179 | 1,052,782 | 3,397 |
| Weighted sample size (100,000s) | 6,522.3 | 6,497 | 25.3 |
| Years | 2014-20 | 2014-20 | 2014-20 |

*Notes:* Sample weights are applied. Percentages are rounded to closest integer. Sample omits states that do not use the sexual orientation gender identity module. We also omit any respondents who do not report their gender identity, household income, age, race, employment, education, or marital status–approximately 2.5% of the sample. Federal poverty level is the ratio of a tax-units (group filing taxes together) total modified-adjusted gross income to size of the tax-unit. Since this information is not available on the BRFSS, we impute the federal poverty level using the 2015-2021 March CPS.

Table 2: First-stage and Reduced Form Estimates

| | (1) Cis | (2) Trans | (3) Cis | (4) Trans | (5) Cis | (6) Trans | (7) Cis | (8) Trans | (9) Cis | (10) Trans | (11) Cis | (12) Trans | (13) Cis | (14) Trans | (15) Cis | (16) Trans |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *First stage* | | | | | | | | | | | | | | | | |
| Age jump: 65 | .052*** | .077** | .052*** | .077** | .053*** | .077** | .052*** | .071** | .052*** | .075** | .052*** | .052* | .05*** | .055 | .052*** | .074** |
| | (.004) | (.035) | (.003) | (.033) | (.004) | (.036) | (.004) | (.032) | (.003) | (.033) | (.003) | (.028) | (.004) | (.036) | (.003) | (.033) |
| Age jump: 26 | −.029*** | −.333*** | −.029*** | −.303*** | −.03*** | −.337*** | −.029*** | −.304*** | −.029*** | −.315*** | −.03*** | −.322*** | −.029*** | −.323*** | −.028*** | −.332*** |
| | (.009) | (.085) | (.009) | (.081) | (.009) | (.088) | (.009) | (.086) | (.009) | (.083) | (.009) | (.083) | (.009) | (.087) | (.009) | (.085) |
| *Reduced Form* | | | | | | | | | | | | | | | | |
| Age jump: 65 | −.146 | −5.1*** | −.557*** | −2.5 | −.24** | −3.98** | −.008** | −.049 | −.023*** | −.014 | −.015*** | −.128** | −.002 | −.201*** | −.011** | −.166*** |
| | (.107) | (1.9) | (.106) | (1.63) | (.11) | (1.93) | (.003) | (.036) | (.003) | (.061) | (.003) | (.06) | (.004) | (.077) | (.004) | (.061) |
| Age jump: 26 | −.24 | 7.11*** | .262** | 3.85** | −.116 | 4.56** | .002 | .087** | .005 | .229** | −.016** | .119 | .008* | .155* | −.015* | .028 |
| | (.192) | (2.19) | (.117) | (1.52) | (.109) | (1.91) | (.003) | (.041) | (.009) | (.115) | (.007) | (.091) | (.004) | (.094) | (.008) | (.104) |
| Outcome | Mental days | Mental days | Physical days | Physical days | Bad days | Bad days | Poor health | Poor health | Cost | Cost | Concen-tration | Concen-tration | Errands | Errands | De-pressed | De-pressed |
| Sample size | 534,731 | 1,678 | 718,858 | 2,218 | 564,987 | 1,729 | 540,537 | 1,712 | 729,428 | 2,266 | 725,476 | 2,257 | 495,200 | 1,550 | 728,361 | 2,265 |
| First-stage F Statistics | 92.4 | 10.1 | 140.9 | 9.7 | 101 | 9.6 | 93.1 | 8.7 | 144.8 | 9.9 | 143.1 | 9.2 | 73.4 | 8.1 | 145.2 | 10.2 |
| Polynomial | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| Kernal | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar | Triangu-lar |
| Bandwidth | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal | MSE-optimal |

*Notes:* Standard errors are clustered by sampling unit and given in parentheses. Sampling weights are applied. *$*p<0.10, **p<0.05, ***p<0.01$.* See Figure 4 *notes* for a description of the benchmark specification, which is used here. `Mental` is the number of days the respondent reports bad mental health over the thirty days prior to the interview. `Physical` is the number of days the respondent reports bad physical health over the thirty days prior to the interview. `Poorhealth` is the number of days the respondent reports bad mental or physical health over the thirty days prior to the interview. `Badhealth` indicates the respondent reported having poor health in general. `Cost` indicates the respondent needed to see a doctor in the past 12 months, but was not able to because of cost. `Concentration` indicates the respondents has serious difficulty concentrating, remembering, or masking decisions because of a physical, mental, or emotional condition. `Errands` indicates the respondents has serious difficulty doing errands because of a physical, mental, or emotional condition. `Depressed` indicates the respondents has been told that they have a depressive disorder.

Table 3: Impact of Health Insurance Coverage on Health Outcomes by Transgender Status and Estimator

| | (1) Cis | (2) Trans | (3) Cis | (4) Trans | (5) Cis | (6) Trans | (7) Cis | (8) Trans | (9) Cis | (10) Trans | (11) Cis | (12) Trans | (13) Cis | (14) Trans | (15) Cis | (16) Trans |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Age Cutoff: 65* | | | | | | | | | | | | | | | | |
| Insurance | .86 | −45.7 | −1.91 | −23.1 | .434 | −26.5 | −.026 | −.274 | −.311*** | .442 | −.026 | −2.55 | .088 | −1.4 | .003 | −2.69* |
| | (2.11) | (28.7) | (2.13) | (20.2) | (2.09) | (22.5) | (.065) | (.565) | (.065) | (.754) | (.068) | (1.83) | (.077) | (1.32) | (.088) | (1.54) |
| First-stage F | 184.5 | 3.9 | 277.3 | 5.4 | 202.9 | 3.7 | 186.2 | 3.9 | 281.8 | 5 | 277.4 | 3 | 144.9 | 3 | 282.5 | 5 |
| Sample size | 420,160 | 1,166 | 604,318 | 1,704 | 451,642 | 1,233 | 424,925 | 1,186 | 613,892 | 1,743 | 610,594 | 1,733 | 380,202 | 1,027 | 613,092 | 1,743 |
| *Age Cutoff: 26* | | | | | | | | | | | | | | | | |
| Insurance | 8.12 | −20.6** | −8.95* | −13.1* | 3.68 | −12.8 | −.067 | −.283 | −.168 | −.698* | .54* | −.38 | −.266 | −.45 | .533 | −.12 |
| | (7.2) | (9.98) | (4.83) | (6.74) | (3.77) | (7.93) | (.103) | (.179) | (.308) | (.381) | (.29) | (.33) | (.165) | (.341) | (.334) | (.354) |
| First-stage F | 10.4 | 13.1 | 11 | 11.3 | 11.3 | 12.3 | 10.4 | 10.7 | 10.4 | 11.8 | 11 | 12.2 | 10.6 | 12.1 | 10.2 | 12.5 |
| Sample size | 114,571 | 512 | 114,540 | 514 | 113,345 | 496 | 115,612 | 526 | 115,536 | 523 | 114,882 | 524 | 114,998 | 523 | 115,269 | 522 |
| *Stacked: 2SLS* | | | | | | | | | | | | | | | | |
| Insurance | −.96 | −23.7** | −10.4*** | −15** | −3.09* | −15.6** | −.143** | −.312* | −.398*** | −.658** | −.142** | −.458 | −.082 | −.561* | −.085 | −.281 |
| | (2.05) | (9.37) | (1.98) | (6.14) | (1.86) | (7.5) | (.057) | (.164) | (.073) | (.322) | (.069) | (.297) | (.068) | (.327) | (.086) | (.317) |
| First-stage F | 92.4 | 10.1 | 140.9 | 9.7 | 101 | 9.6 | 93.1 | 8.7 | 144.8 | 9.9 | 143.1 | 9.2 | 73.4 | 8.1 | 145.2 | 10.2 |
| Sample size | 534,731 | 1,678 | 718,858 | 2,218 | 564,987 | 1,729 | 540,537 | 1,712 | 729,428 | 2,266 | 725,476 | 2,257 | 495,200 | 1,550 | 728,361 | 2,265 |
| *Stacked: LIML* | | | | | | | | | | | | | | | | |
| Insurance | −.97 | −25.8** | −10.4*** | −15.4** | −3.14* | −17.2** | −.144** | −.317* | −.399*** | −.661** | −.153** | −.495 | −.083 | −.679 | −.09 | −.348 |
| | (2.1) | (10.6) | (1.98) | (6.39) | (1.9) | (8.49) | (.057) | (.168) | (.074) | (.325) | (.076) | (.324) | (.069) | (.413) | (.089) | (.379) |
| First-stage F | 92.4 | 10.1 | 140.9 | 9.7 | 101 | 9.6 | 93.1 | 8.7 | 144.8 | 9.9 | 143.1 | 9.2 | 73.4 | 8.1 | 145.2 | 10.2 |
| Sample size | 534,731 | 1,678 | 718,858 | 2,218 | 564,987 | 1,729 | 540,537 | 1,712 | 729,428 | 2,266 | 725,476 | 2,257 | 495,200 | 1,550 | 728,361 | 2,265 |
| Outcome | Mental days | Mental days | Physical days | Physical days | Bad days | Bad days | Poor health | Poor health | Cost | Cost | Concen-tration | Concen-tration | Errands | Errands | De-pressed | De-pressed |

*Notes:* Standard errors are clustered by sampling unit and given in parentheses. Sampling weights are applied. **p<0.10, **p<0.05, ***p<0.01.* See Figure 4 *notes* for a description of the benchmark specification, which is used here. `Mental` is the number of days the respondent reports bad mental health over the thirty days prior to the interview. `Physical` is the number of days the respondent reports bad physical health over the thirty days prior to the interview. `Poorhealth` is the number of days the respondent reports bad mental or physical health over the thirty days prior to the interview. `Badhealth` indicates the respondent reported having poor health in general. `Cost` indicates the respondent needed to see a doctor in the past 12 months, but was not able to because of cost. `Concentration` indicates the respondents has serious difficulty concentrating, remembering, or masking decisions because of a physical, mental, or emotional condition. `Errands` indicates the respondents has serious difficulty doing errands because of a physical, mental, or emotional condition. `Depressed` indicates the respondents has been told that they have a depressive disorder.